



**Fermi National Accelerator Laboratory**

**FERMILAB-Conf-93/282**

**Panel Session— Part I**  
**In Flux—Science Policy and the Social Structure of Big**  
**Laboratories, 1964-1979**

**Catherine Westfall**

*Michigan State University  
CEBAF, Newport News, Virginia  
Fermilab History Collaboration, P.O. Box 500, Batavia, Illinois 60510*

**Panel Session—Part II**  
**Some Sociological Consequences of High Energy**  
**Physicists' Development of the Standard Model,**  
**1964-1979**

**Mark Bodnarczuk**

*National Renewable Energy Laboratory  
The University of Chicago  
Fermilab History Collaboration, P.O. Box 500, Batavia, Illinois 60510*

**September 1993**

To be published in the *Proceedings of the Third International Symposium on the History of Particle Physics: the Rise of the Standard Model*, SLAC, June 1992

## **Disclaimer**

*This report was prepared as an account of work sponsored by an agency of the United States Government. Neither the United States Government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of any information, apparatus, product, or process disclosed, or represents that its use would not infringe privately owned rights. Reference herein to any specific commercial product, process, or service by trade name, trademark, manufacturer, or otherwise, does not necessarily constitute or imply its endorsement, recommendation, or favoring by the United States Government or any agency thereof. The views and opinions of authors expressed herein do not necessarily state or reflect those of the United States Government or any agency thereof.*

**Panel Session – Part I**  
**In Flux – Science Policy and the Social Structure of Big**  
**Laboratories, 1964-1979**  
Catherine Westfall  
*Michigan State University*  
and  
*Continuous Electron Beam Accelerator Facility*  
and  
*Fermilab History Collaboration*

The era that witnessed the rise of the Standard Model also saw radical change in the science policy and sociology of large laboratories. In the fifteen year span from 1964 to 1979 the science policy climate in Europe and the U.S. evolved from the post-World War II golden age of strong political support and burgeoning budgets to the current era of political vacillation and uncertain funding. As researchers investigating the fundamental nature of matter used fewer mammoth accelerators and larger, vastly more complicated detectors requiring larger teams and more specialized workers, the social structure of large laboratories also was transformed.

To help illuminate this pivotal era, the conference organizers convened a panel on the Science Policy and Sociology of Big Laboratories. I chaired the panel, which included two other historians specializing in big science (Robert Seidel and John Krige), philosopher of science Mark Bodnarczuk, and four physicists who helped administer laboratories during these years (William Wallenmeyer, Wolfgang (Pief) Panofsky, Maurice Goldhaber, and Norman Ramsey). The panel session, which consisted of 15-minute presentations by each panel member followed by a brief discussion period, was videotaped. Panofsky and Goldhaber also gave me written remarks. At the request of the conference organizers, I reviewed the videotape and written remarks and integrated, expanded, and placed into context common themes from the panel discussion to create this essay. Panelists are quoted from the videotape of the panel session or from their texts, as indicated. In a few cases, as noted, I quote relevant remarks made by panelists on other occasions. Comments not attributed to other panel members reflect my own interpretations.

While writing this essay I found that Bodnarczuk's comments drew on specialized concepts and language particular to the philosophy and sociology of science, fields that are outside my specialty. Since Bodnarczuk alone

addressed the issues currently studied by sociologists of science— a crucial task for a panel covering the sociology of big laboratories - I felt obliged to present his views completely and accurately. Since I was uncertain that I could accomplish this goal on my own, I asked him to write a separate essay based on his panel contribution. His essay follows mine.

The first section of my essay charts the evolution of the relationship between large laboratories and government while the second section describes changes in laboratory administration and research. The essay ends with some reflections on the future of large laboratories in light of the trends evident in the 1964 to 1979 period. My intention is twofold: to present fresh information and provide a point of departure for further scholarly investigation.

### **The Partnership in Crisis**

Panelists agreed that in the two decades after World War II large laboratories and their government sponsors collaborated in a close “partnership” to accomplish mutually beneficial goals. In the words of former Stanford Linear Accelerator Center (SLAC) director Panofsky, this partnership “worked exceptionally well.” Panelists disagreed, however, about the terms of the partnership. Panofsky argued that: “The relationship was based on the recognition of a commonality of interests.... During World War II government found that, if adequately funded, physicists are very productive.”<sup>1</sup> In Wallenmeyer’s words, both partners “expected a payoff” from the federal investment in both pure and applied research dividends, although “there was no way of knowing when or how this payoff would occur.” Wallenmeyer added that the government also supported large physics laboratories “in recognition of the wartime contribution made by physicists” and because officials felt that large-scale physics research was so expensive that “the federal government was the most appropriate source of funding.”<sup>2</sup> Krige and Seidel identified other motives for federal support. Seidel insisted that the U.S. government was motivated, primarily by national security objectives, to sponsor the research of large laboratories, in particular the development of accelerators, to increase international prestige as well as recruit personnel

---

<sup>1</sup>Wolfgang Panofsky, “Round Table Statement,” submitted to the Panel on Science Policy and Sociology of Big Laboratories.

<sup>2</sup>Panel session.

and develop technology for applied, especially military, projects. The close connection Seidel finds between the development of accelerators and national security, in his words, “supports the arguments of Daniel Kevles, and others, who note that big science originated in the alliance made between the Army and prominent physicists during the Manhattan Project. Since that time, this argument maintained, the scientific elite has accomplished its goals through ties with the military and other power elites.”<sup>3</sup> Krige concluded that European governments did not support large laboratories only for military reasons. They also had scientific, and political motives and wished to bridge the gap between European and U.S. research capabilities and thereby halt and redress the brain drain from the continent to the U.S.

Although panelists disagreed about the post World War II “golden age,” as Seidel called it, they concurred that the partnership between large laboratories and their governments began to change rapidly in the mid-1960s.<sup>4</sup> By the early 1980s, they agreed, the partners had fewer common interests, less trust, and less contact.

The experience at large laboratories reflected changes encompassing all of federally sponsored research. Bruce Smith, Jeffrey Stine, David Dickson and other science policy analysts report that from the mid-1960s to the mid-1970s a number of factors prompted a “crisis,” which transformed the relationship between government and science.<sup>5</sup> By the mid-1960s, public complaint about the highly technological war in Vietnam, the development of civilian nuclear power, and environmental pollution prompted politicians to debate the social value of science. In this critical atmosphere, skepticism rose about the role of scientists in policy making. In September 1963, for example, U.S. political reporter Meg Greenfield remarked: “As presiders over the national purse, are the scientists speaking in the interest of science ... government or ...

---

<sup>3</sup>Robert Seidel, “Summary of Symposium on Science Policy Issues of Large National Laboratories,” submitted to the Institute of Government and Public Affairs, University of Illinois 8 October 1992.” See Daniel Kevles, “K<sub>1</sub>S<sub>2</sub>: Korea, Science, and the State,” in Peter Galison and Bruce Hevly, *Big Science: The Growth of Large Scale Research* (Stanford: Stanford University Press, 1992).

<sup>4</sup>Panel session.

<sup>5</sup>Jeffrey K. Stine, *A History of Science Policy in the United States, 1940-1985*, Committee on Science and Technology, House of Representatives, 99th Congress, Second Session (Washington D.C.: GPO, 1986), pp. 57-58; Bruce L. R. Smith, *American Science Policy Since World War II*, (Washington D.C.: Brookings Institution: 1990), pp. 73-118; David Dickson, *The New Politics of Science*, (Chicago: The University of Chicago Press, 1984).

their own institutions? Is their policy advice ... offered in furtherance of national objectives -- or agency objectives -- or their own objectives?"<sup>6</sup> By late 1963 Congressional investigations were being formed and by mid-decade various aspects of science and technology funding were under close scrutiny.<sup>7</sup> In Europe, scientists were also under fire. As Krige noted, in the midst of "general public disillusionment about the role scientists ... European policy makers were simply no longer willing to accept the claims of scientists on faith."<sup>8</sup>

Political differences caused further divisions between leaders of the scientific community and top government officials. For example, eminent scientists, including members of the prestigious President's Science Advisory Committee (PSAC), vehemently opposed U.S. President Richard Nixon's plans to develop antiballistic missiles and supersonic transport. This reaction annoyed Nixon, who was already irritated because many scientists opposed the Vietnam War. Although a 1970 House Subcommittee headed by Emilio Dad-

---

<sup>6</sup>As quoted in Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America*, (New York: Alfred Knopf, 1978), p. 395.

<sup>7</sup>Committees that investigated the appropriate distribution of research funding included a House subcommittee headed by Emilio Daddario and a House select committee headed by Carl Elliot, both established in 1963, a Senate committee headed by Joseph S. Clark convened in 1965 and a Senate subcommittee headed by Fred R. Harris, established in 1966. See U.S. Congress, Subcommittee of the Select Committee on Small Business, *The Role and Effect of Technology in the Nation's Economy*, 88th Congress, First Session, (Washington D.C.: GPO, 1963); U.S. Congress, *Government and Science: Hearings Before the Subcommittee on Science, Research, and Development of Committee on Science and Astronautics*, 88th Congress, Second Session (Washington D.C.: GPO, 1964); U.S. Congress, Subcommittee on Science, Research and Development of the Committee on Science and Astronautics, *Government and Science: Distribution of Federal Research Funds*, 88th Congress, Second Session (Washington D.C.: GPO, 1964); U. S. Congress, Subcommittee on Employment and Manpower of the Committee on Labor and Public Welfare, *Impact of Federal Research and Development Policies on Scientific and Technical Manpower*, 89th Congress, First Session (Washington D.C.: GPO, 1965); and U.S. Congress, Subcommittee on Government Research of the Committee on Government Operations, Hearings, *Equitable Distribution of R&D Funds by Government Agencies*, 89th Congress, Second Session, (Washington D.C.: GPO, 1966). Also see Michael D. Reagan, *Science and the Federal Patron*, (New York: Oxford University Press, 1969); Donald R. Fleming, "The Big Money and High Politics of Science," *Atlantic Monthly*, August, 1965; and Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America*, pp. 413-414.

<sup>8</sup>Panel session.

dario and a White House Task Force recognized the escalating divisiveness between the scientific community and government and advocated increasing the number and influence of science advisory officers, Nixon abolished PSAC and the Office of Science and Technology and gave the role of science advisor to H. Guyford Stever, who simultaneously directed the National Science Foundation (NSF). The executive branch science advisory system was not reinstated until 1977.<sup>9</sup>

Other forces conspired to widen the gap between large laboratories and government. As Krige explained, in the 1960s and 1970s Europe lost, due to death or retirement, Francis Perrin, Werner Heisenberg, and other top physicists instrumental to the post World War II campaign to revitalize European science. Their successors had less political experience and fewer close ties to government leaders and therefore enjoyed less influence in government. One early consequence was the failure of British physicists to forestall Britain's attempt to enforce budget ceilings at the European high energy physics laboratory, CERN, in the early 1960s. Without an eminent spokesman "the process of policy formation inside Britain was highly bureaucratized: the mechanisms used by the physicists to transmit their views on CERN to the government were predominantly formal, and so inevitably lacked 'punch' and a sense of urgency."<sup>10</sup>

In the U.S. institutional changes complicated the administration of large laboratories and further decreased communication between laboratory officials and government leaders. Prompted by the concerns for promoting new,

---

<sup>9</sup>Jeffrey K. Stine, *A History of Science Policy in the United States, 1940-1985*; Bruce L. R. Smith, *American Science Policy Since World War II*, pp. 73-118; Thaddeus Trenn, *America's Golden Bough: The Science Advisory Intertwist* (Cambridge: Oelgeschlager, Gunn, and Hain, 1983, p. 88-112. For more information on the 1970 Daddario Hearings, see U.S. Congress, Subcommittee on Science and Astronautics, Subcommittee on Science, Research and Development, *Toward a Science Policy for the United States*, 91st Congress, Second Session (Washington, D.C.: GPO, 1970). For further discussion on the ABM debate and a review of other key decisions made by the science advisory system, see Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI*, (New York: Oxford University Press: 1992) and Bruce L. R. Smith, *The Advisor: Scientists in the Policy Process*, (Washington, D.C.: The Brookings Institution, 1992).

<sup>10</sup>John Krige, "Finance Policy: The Debates in the Finance Committee and the Council Over The Level of the CERN Budget," in Armin Hermann, John Krige, Ulrike Mersits, Dominique Pestre, *History of CERN: Building and Running the Laboratory, 1954-1965*, Vol. II (Amsterdam: North-Holland, 1990) pp. 602-603. This source contains details on the dispute and explains the resulting budget policy.

non nuclear energy sources and for separating nuclear development from nuclear safety, President Gerald Ford in 1974 abolished the Atomic Energy Commission (AEC), which had supported the nation's largest accelerators since its formation in 1946. The AEC's research and development function was transferred to the newly formed Energy Research and Development Administration, which brought together, for the first time, major research and development programs for all types of energy.<sup>11</sup> In 1977 ERDA was reorganized into a cabinet level Department of Energy (DOE). Wallenmeyer, former Director of the Division of High Energy Physics, explained that as the funding agency got larger, accelerator laboratories had to compete for funding with a wider range of programs. Also, with size came "greater bureaucracy and less scientific and technical understanding" at the higher levels of the agency.<sup>12</sup> As a result, laboratory directors had to adhere to more regulations, and produce more paperwork to account for their activities. In 1977, in the midst of the transition from the AEC to DOE, the 30 year old Joint Committee on Atomic Energy (JCAE) was disbanded. Thereafter budget items were considered by established Congressional committees instead of by the JCAE, which had included members well versed in science and technology who were willing to champion deserving accelerator projects through the Congressional funding process.<sup>13</sup>

Ramsey, who helped plan Fermi National Accelerator Laboratory (Fermilab) as president of the Universities Research Association (URA), explained the challenges faced by laboratory administrators during this period. "In the late 1960s we could go to the top – to powerful Congressmen and even to President Johnson through Glenn Seaborg, who was a scientist and had worked with people like Robert Wilson and Pief Panofsky. But none of us knew the top echelons of DOE," in the 1970s, "and we had less contact with Congress after the JCAE left. With less contact, communication, and

---

<sup>11</sup>In response to criticism that a single agency should not administer and regulate atomic energy programs, Ford assigned regulatory functions to a separate organization, the Nuclear Regulatory Commission. An Energy Resources Council was also established at this time. A. L. Buck, *A History of the Energy Research and Development Administration*, (Washington, D.C.: Department of Energy, 1982), p. 2.

<sup>12</sup>Panel session.

<sup>13</sup>A. L. Buck, *A History of the Energy Research and Development Administration*, p. 14; "Congressional Science Committees Have a New Look," *Physics Today*, 30, May 1977, p. 109.



understanding, problems were harder to solve and life got more difficult.”<sup>14</sup> Problem solving was further complicated, he noted, by the delays induced by greater bureaucracy in Washington and the greater size and complexity of laboratory projects. For example, when Ramsey helped plan Brookhaven National Laboratory (BNL), including a proposal for a \$25 million research reactor, only 14 months elapsed between planning sessions in late 1945 and the beginning of work at the new laboratory in early 1947.<sup>15</sup> In contrast, when planning began for the \$250 million Fermilab accelerator in 1959, 12 years passed before staff members went to work at the new accelerator site. “With greater delay came greater uncertainty – maintaining morale was a real challenge.”<sup>16</sup>

As the gap widened between physics and government, national economies worsened both in the U.S. and in Europe; research budgets soon fell victim to the times. As Seidel noted, although physics had enjoyed an almost exponential increase in funding in the U.S. from 1945 to 1967, in 1968 funding reached a plateau. This plateau continued with decreases in the mid-1970s and early 1980s. (See Figure 1.)<sup>17</sup> European research also suffered. The United Kingdom, France, and West Germany reduced research and development expenditures (as a percentage of gross national product) in the late 1960s. Although funding in West German increased steadily through the 1970s, the investment slump continued during this period in the United King-

---

<sup>14</sup>Catherine Westfall interview with Norman Ramsey, 13 September 1985, Fermilab History Collection, Batavia, Illinois.

<sup>15</sup>Allan Needel, “Nuclear Reactors and the Founding of Brookhaven National Laboratory,” *Historical Studies in the Physical Science*, 14, 1983, p. 119. For more information on the founding of BNL, see Norman Ramsey, “Early History of Associated Universities and Brookhaven National Laboratory,” BNL 992 (T-421), Upton, March 1966.

<sup>16</sup>Panel session. The first plans for a multi-hundred GeV cascade synchrotron were made by Matthew Sands in 1959. For more information on this design, as well as earlier designs for other large synchrotrons, see Catherine Westfall, “The First ‘Truly National Accelerator’: The Birth of Fermilab,” Ph.D. Dissertation, Michigan State University, 1988; Lillian Hoddeson, “Establishing KEK in Japan and Fermilab in the US: Internationalism, Nationalism and High Energy Accelerator Physics During the 1960s,” *Social Studies of Science*, 13 (1983), pp. 1-48.

<sup>17</sup>Spencer Weart, “The Physics Business in America, 1919-1940: A Statistical Reconnaissance,” in Nathan Reingold, ed., *The Sciences in the American Context: New Perspectives* (Washington D.C.: Smithsonian Institution Press, 1979), p. 327; Physics Survey Committee, “Organization and Support of Physics,” *Physics Through the 1990s: An Overview*, (Washington, D.C.: National Academy Press, 1986), pp. 119-120.

dom and France. (See Figure 2.)<sup>18</sup>

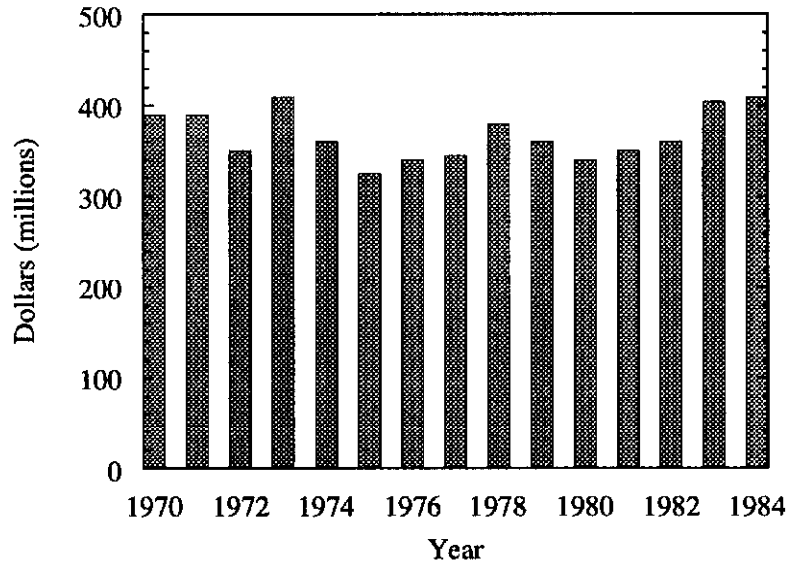


Figure 1: High energy physics funding in the U.S. for fiscal years 1969 to 1984. Funding is given in fiscal year 1983 dollars using selected inflation factors. Construction funds are not included. Data from Physics Survey Committee, *Physics Through the 1990s: An Overview*, (Washington D.C.: National Academy Press, 1985), p. 125.

High energy physics was burdened with several disadvantages during this period. As budgets shrank, expenses rose for the larger detectors and accelerators needed to advance the field. To make matters worse, proposed high energy physics projects had to compete with proposals for space science and other large projects of unprecedented expense.<sup>19</sup> Also, as Seidel and Krige noted, at a time when U.S. and European politicians promoted the value of socially useful research, high energy physics proposals were at a competitive disadvantage because the field promised few immediate practical applications. Large U.S. projects faced further difficulties. The largest

<sup>18</sup>Physics Survey Committee, *Physics Through the 1990s: An Overview*, p. 77.

<sup>19</sup>For a description of one such U.S. project, see Robert W. Smith, *The Space Telescope: A Study of NASA, Science Technology, and Politics*, (Cambridge: Cambridge University Press, 1989).

funding requests, for example the \$250 million proposal for the Fermilab accelerator, were expensive enough to attract considerable public and Congressional attention. Also, as Wallenmeyer noted, “since World War II the AEC and its successors have provided about 90% of the funding for high energy physics.”<sup>20</sup> Thus, even smaller expenses, such as accelerator upgrades like PEP at SLAC and the Energy/Saver Doubler at Fermilab, appeared in a single budget and were therefore more noticeable and vulnerable to budget cuts.

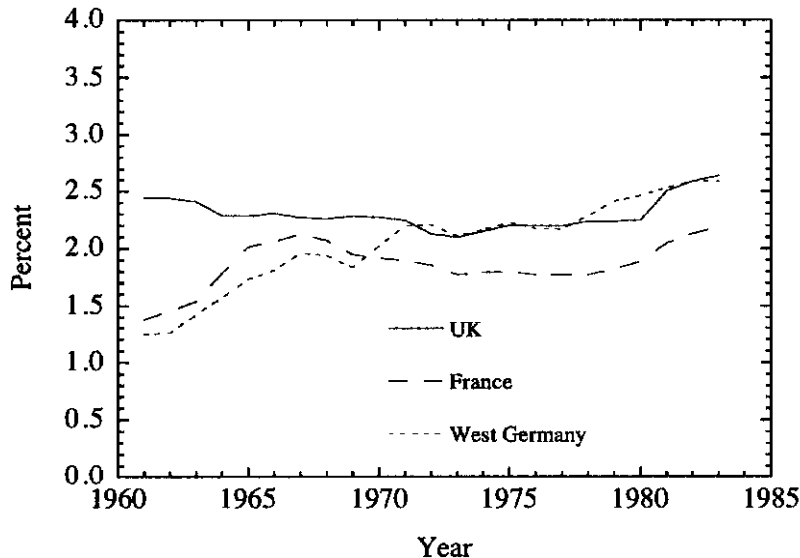


Figure 2: National expenditures for R &D as a percentage of GNP for the United Kingdom, France and West Germany, 1961 - 1985. Data from Physics Survey Committee, *Physics Through the 1990s: An Overview*, (Washington D.C.: National Academy Press, 1985), p. 77.

The disadvantageous funding environment left its mark in both Europe and the U.S. Krige noted that by the late 1970s, “Britain closed several large facilities, including NINA and Nimrod, France formally announced it would no longer build high energy physics facilities, and Italy stopped building accelerators. This left Germany as the sole European country that supported

<sup>20</sup>Panel session.

both CERN and a major national laboratory.” CERN was also affected. “The laboratory was forced to reduce its budget by 3.5% in real terms per year and plans to build the 300 GeV Super Proton Synchrotron (SPS) were delayed due to budgetary concerns.”<sup>21</sup>

U.S. laboratories also felt the pinch of the shrinking budget. In early 1964, President Lyndon Johnson denied funding for the Fixed Field Alternating Gradient accelerator proposed by the Midwestern University Research Association, a group that had been developing highly innovative accelerator ideas, such as colliding beams, since 1954. By 1972, the Princeton-Pennsylvania Accelerator (PPA) was closed and the Cambridge Electron Accelerator (CEA) was no longer used for high energy physics research.<sup>22</sup>

Panofsky judged that the era which saw the development of the Standard Model was “the most creative” period in high energy physics.<sup>23</sup> The intellectual achievements of the time demonstrated that high energy physicists successfully exploited available resources, despite the deteriorating relationship with government, tight budgets, and the escalating size and expense of apparatus. In addition, they built the necessary facilities to facilitate future research. As Krige noted, for the first time since World War II European high energy physicists had the institutions, research expertise, and administrative experience (including procedures for multi-national cooperation), necessary to build, maintain, and efficiently use big science facilities. With these resources, Europeans laid the foundation for an experimental program that would challenge U.S. hegemony in high energy physics. “The Deutsches Elektronen Synchrotron (DESY) began operation of the Double Ring Storage collider (DORIS) in 1972 and received funding to build the Positron-Electron Tandem Ring Accelerator (PETRA) in 1975. At CERN, the Intersecting Storage Ring (ISR) reached design luminosity in 1972, the first successful beam cooling experiments were performed at the SPS in 1977, and approval was obtained for constructing the Large Electron Positron facility (LEP) in

---

<sup>21</sup>Panel session. For more information on the delay in building the SPS, see Dominique Pestre, “The Second Generation of Accelerators for CERN, 1956-1965: The Decisionmaking Process,” in Armin Hermann, John Krige, Ulrike Mersits, Dominique Pestre, *History of CERN: Building and Running the Laboratory, 1954-1965* Vol. II.

<sup>22</sup>CEA was thereafter used for research into the development of colliding beams. Physics Survey Committee, *Physics In Perspective* Vol. II, (Washington D.C.: National Academy of Science, 1972), p. 118.

<sup>23</sup>Panel session.

1979.”<sup>24</sup>

Due to the enduring influence of leaders who successfully adapted to the challenges of the era, U.S. high energy physicists also constructed the equipment needed to advance research. Fermilab received construction authorization in 1967, thanks to support of the still extant JCAE and AEC and to the vigorous efforts of such leaders as Seaborg, Ramsey, and Frederick Seitz, who consolidated support in Washington and within the physics community for the expensive project. Wilson managed to finish the machine ahead of time and under budget, despite delayed funding allocations, using a U.S. accelerator building style dating back to the 1930s that de-emphasized reliability and solid engineering and celebrated frugal, quickly implemented, clever solutions to technical problems. As Ramsey joked, the accelerator, which used small, risky main ring magnets, broke “both the energy and the cost frontier.”<sup>25</sup>

Building the Stanford Positron-Electron Asymmetric Rings (SPEAR) required both frugality and creative financing. As Panofsky explained, from 1965 to 1970 plans to build the “pioneering storage ring ... as a formal capital equipment project or construction project” fell prey “to budgetary pressure.” SLAC was able to build the collider after Burton Richter drastically cut construction costs, AEC Comptroller John Abbadessa “gave informal acquiescence” to the idea of reallocating “ongoing equipment and operating funds,” and Panofsky freed the necessary money by “internal belt-tightening.”<sup>26</sup>

Wallenmeyer argued that funding agency administrators also faced a difficult task when struggling to manage a successful U.S. high energy physics program. “Administering the program was a juggling act that got harder as budgets tightened and projects got larger. We had to balance the needs of universities versus the laboratories, the needs of each laboratory versus the other laboratories. At the same time, we had to balance the well-being of the

---

<sup>24</sup>John Krige, “High Energy Physics Chronology,” 13 May 1992.

<sup>25</sup>Catherine Westfall interview with Norman Ramsey, 13 September 1985, Fermilab History Collection, Batavia, Illinois. For more information see note 15. For more information on the affect of fiscal stringency on the building of Fermilab, see Catherine Westfall and Lillian Hoddeson, “Frugality and the Building of Fermilab, 1960-1972,” Fermilab-Pub-93/283, Batavia, September 1993.

<sup>26</sup>Wolfgang Panofsky, “Round Table Statement.” For a more detailed description of attempts to fund SPEAR, see Michael Riordan, *The Hunting of the Quark: A True Story of Modern Physics*, (New York: Simon & Schuster, Inc.: 1987), pp. 247-248.

current programs, such as the effective operation of existing facilities, future requirements, including R&D for new detectors and accelerators, and R&D on the advanced concepts needed for accelerator development in the very long term future.”<sup>27</sup> Despite budgetary difficulties, SLAC and Fermilab began operation and two older facilities, the Alternating Gradient Synchrotron (AGS) at BNL and the electron synchrotron at Cornell, were maintained. In addition, other major projects were started, including SPEAR and PEP at SLAC, CESR at Cornell, and the Energy Saver/Doubler and the Colliding Detector Facility (CDF) at Fermilab. Wallenmeyer noted that the continued vitality of the U.S. high energy physics program derived in part from the tradition of long-range planning that began in the 1950s with ad hoc advisory panels and culminated with the 1967 formation of the High Energy Physics Advisory Panel (HEPAP), a standing committee of top physicists that judged high energy physics projects and made funding recommendations.<sup>28</sup> With the help of HEPAP, “which is known as the most powerful advisory group in Washington, the funding agencies were able to effectively set priorities and lobby for important projects.”<sup>29</sup>

## A New Social Order

Panelists also remarked on the transformation in the social structure of laboratory life that coincided with the new science policy environment. The increase in the scale of research, plus limited budgets, led to radical alterations in laboratory administration and experimentation. In short, in Bodnarczuk’s words, “what it meant to do high energy physics changed forever.”<sup>30</sup>

---

<sup>27</sup>Panel session.

<sup>28</sup>High Energy Physics Advisory Committee, “Minutes of HEPAP Organizing Meeting,” 29 January 1967, FNAL. As Wallenmeyer noted, high energy physics advisory panels included: a 1954 NSF Panel chaired by Robert Bacher; a 1956 and a 1958 NSF panel, both chaired by Leland Haworth; a 1958 and a 1960 PSAC-General Advisory Panel (GAC), both chaired by Emanuel Piore, and a 1963 PSAC-GAC Panel chaired by Norman Ramsey. For copies of panel reports, see Joint Committee on Atomic Energy, *High Energy Physics Program: Report on National Policy and Background Information*, (Washington, D.C.: GPO, 1965).

<sup>29</sup>Catherine Westfall interview with William Wallenmeyer, 18 October 1992, Continuous Electron Beam Accelerator Facility Archive, Newport News, Virginia.

<sup>30</sup>Panel session. Bodnarczuk addresses the increase in the scale of experimentation in

Numerous administrative changes resulted from a shift in the relationship between laboratories and outside users, which began in the 1950s. At this time, numerous small accelerators, mostly cyclotrons, closed as interest shifted to the research capabilities of larger, more expensive machines, such as the AGS at BNL, SLAC at Stanford, and the Proton Synchrotron (PS) at CERN.<sup>31</sup> As physicists congregated at a dwindling number of facilities, complaint rose about the treatment of outside users. As Panofsky pointed out, this caused a major problem for laboratory administrators, who realized that no laboratory can live up to its research potential without a large group of enthusiastic, well-motivated users.<sup>32</sup>

Goldhaber explained that BNL pioneered early efforts to accommodate the entire community of users. After the Cosmotron began operation in the early 1950s, Leland Haworth gathered BNL physicists and “occasional outsiders” to form a formal program committee, “a concept which seems,” in Goldhaber’s words “to have originated at Brookhaven.” To further facilitate the fair treatment of outside users, in 1961 Goldhaber “reconstituted” an existing advisory committee that judged proposals for AGS experiments “to contain comparable numbers of high-energy physicists from Brookhaven and from neighboring universities” in an effort to “balance different interests” in the advisory process, creating a model for the modern program advisory committee.<sup>33</sup> According to Wallenmeyer, Edwin Goldwasser pioneered other attempts to accommodate outside users in the U.S. During the construction of the Zero Gradient Synchrotron at Argonne in the late 1950s, Goldwasser organized a users group so that outside users could discuss their common concerns and express these concerns to laboratory administrators. In the next

---

## Part II.

<sup>31</sup>For example, from 1958 to 1969 the number of U.S. high energy physics accelerators was reduced by more than a half, from 15 to 7. Appendix 3, Joint Committee on Atomic Energy, *High Energy Physics Program: Report on National Policy and Background Information*,; Physics Survey Committee, *Physics Through the 1990s: An Overview*, pp. 126-127.

<sup>32</sup>Panofsky noted that although Dubna’s 10 Synchrophasotron was the most powerful accelerator in the world from 1957 to 1959, it produced few results. In addition to design problems which hampered machine performance, the accelerator had too few users. The electron positron collider in Beijing could well have faced the latter problem if U.S. physicists had not stressed the importance of early cooperation with users.

<sup>33</sup>Maurice Goldhaber, “The Beginning of Program Committees,” submitted to the Panel on Science Policy and Sociology of Big Laboratories.

few years, users groups and program advisory committees became standard at U.S. accelerator laboratories. As Krige noted, by virtue of its international character, CERN was forced to respond to outside user concerns. The laboratory set up a number of experimental committees based on technique (emulsion, bubble chambers, electronics) “in which visitors were well-represented,” and devised procedures for equitable access to experimental resources, such as beams and bubble chamber photographs.<sup>34</sup>

Despite such efforts, outside user discontent intensified in the mid-1960s and early 1970s when tightening budgets forced the closure of national laboratories in Europe and major U.S. laboratories, such as PPA. Lew Kowarski, who surveyed users procedures, identified the problem in a 1967 CERN report. “Practically every accelerator Laboratory has been originally set up in a framework more narrow than the Commonwealth of users it ultimately came to serve.” As a result, even those laboratories “which have been set up from the start as co-operative,” such as BNL and CERN, had to devise new procedures to ensure that users from institutions outside the framework had equitable access to laboratory resources, including accelerator time.<sup>35</sup>

To forestall outside user discontent, SLAC’s contract specified that the laboratory would form a scientific policy committee to assure fair access to the accelerator, which began operation in 1966.<sup>36</sup> “The growing, grass roots movement for outside user rights,” as Leon Lederman later called it, had an even more profound affect on Fermilab.<sup>37</sup> When Lawrence Radiation Laboratory (LRL) physicists received design funding for the new accelerator in 1963, they assumed they would enjoy the traditional prerogative of accel-

---

<sup>34</sup>For more information on CERN, see John Krige, “The Relationship Between CERN and its Visitors in the 1970s,” to appear in John Krige (ed.), *History of CERN, 1965-1980*, Vol. III (Amsterdam: North Holland, 1994) and Dominique Pestre, “The Organization of the Experimental Work Around the Proton Synchrotron, 1960-1965: the Learning Phase,” Armin Hermann, John Krige, Ulrike Mersits, Dominique Pestre, *History of CERN: Building and Running the Laboratory, 1954-1965*, Vol. II.

<sup>35</sup>Lew Kowarski, “An Observer’s Account of User Relations in the U.S. Accelerator Laboratories,” CERN 67-4, Geneve, January 1967, p. 3.

<sup>36</sup>Richter argues that SLAC felt less pressure from outside users than Fermilab or CERN because fewer people were interested in lepton than proton physics and because initial SLAC experiments clearly required large-scale equipment, which was more easily planned and built by large, in-house groups. Catherine Westfall interview with Burton Richter, 24 June 1992.

<sup>37</sup>Catherine Westfall interview with Leon Lederman, 20 July 1984.



ator builders to manage and build the machine at the site of their choice. Instead, worry that LRL would follow its traditional practice of allowing insiders to monopolize the machine led to the formation of the URA, the first accelerator management consortium with nation-wide representation, and an open, AEC-sponsored site contest, which located the machine in Illinois.<sup>38</sup> After his 1967 appointment as director, Wilson chose outside user expert Goldwasser as deputy director, vowed that the laboratory would be “sensitively responsive to the needs of the broad community of scientists,” and promised that laboratory physicists would conduct only 25% of the research performed on the new accelerator.<sup>39</sup>

Complaints also surfaced at CERN. As Krige explained, during a series of meetings held by the European Committee for Future Accelerators (ECFA) in the early 1970s, CERN’s visitors complained “that the resources and facilities for European high-energy physics were becoming concentrated at CERN” and “this concentration of resources was going along with a concentration of privileges for the in-house staff...” In their view CERN staff members had higher pay, more job security, better working conditions, more decision making power, and obtained funds more readily for experimental equipment than did visitors. To ease such concerns, CERN in 1970s and early 1980s formed the Advisory Committee for CERN Users, studied decision making procedures, and surveyed users’ attitudes.<sup>40</sup>

When the relationship between U.S. laboratories and users changed in the mid-1960s and early 1970s, other aspects of laboratory administration were affected. Wallenmeyer noted that the increased influence of outside users, through users groups and laboratory committees, amplified the voice of universities in laboratory decision making, since most outside users came from universities. “This was very useful because laboratories got the benefit of university leadership and the ties between universities and laboratories got stronger, which was good, since closer collaboration was needed as experi-

---

<sup>38</sup>The URA was modeled on the Associated Universities Incorporated, the regional consortium of universities that manages BNL. For more information on this episode, see Catherine Westfall, “The Site Contest for Fermilab,” *Physics Today*, 42, pp. 44-52 (1989).

<sup>39</sup>National Accelerator Laboratory, “Design Report,” (Batavia: National Accelerator Laboratory, 1968), pp. 2-5 and 3-11.

<sup>40</sup>For more information on this episode, see John Krige, “The Relationship Between CERN and its Visitors in the 1970s,” to appear in John Krige (ed.), *History of CERN, 1965-1980*, Vol. III (Amsterdam: North Holland, 1994).

ments became longer and more expensive.”<sup>41</sup>

Other administrative changes of the era were greeted with less enthusiasm, since the measures that ensured fair decisions in the 1960s and 1970s also made the decision making process more formal and less flexible. Before the advent of formal program committees, laboratory directors often met promising researchers in the early stages of experimental planning and suggested modifications, perhaps with the help of a few trusted advisors. The obligations of experimenters and their institutions were agreed upon with a handshake. As Fermilab researcher Thomas Kirk noted in 1970: “The confidential nature of the proceedings avoided unnecessary embarrassment to experimenters .... Very casual proposals were accepted on the reputations of the men responsible.” As Goldhaber noted, in later times “funding agencies ... sometimes made a grant to a research group only *after* their experimental proposal had been accepted by a committee.” Thus, capable experimenters sometimes faced “the deep psychological impact” of a proposal failed due to some easily corrected flaw, and other, less capable researchers were allowed to construct costly apparatus, thus obtaining “experiments with tenure.”<sup>42</sup>

Outside participation in decision making was not the only factor that increased formality: procedures for processing experimental proposals became increasingly elaborate throughout the 1970s in response to the escalating scale of detectors, which was spurred by the development of the Standard Model, and the decreased technical understanding and trust of funding agencies. For example, by the late 1970s Fermilab had a handbook for users that described the decision making procedures for proposals, including the roles and responsibilities of decision makers, and “Agreements,” which described the obligations and expectations of the institutions involved in experiments.<sup>43</sup>

Increase in scale had other consequences for experimentation. The formation in the late 1970s of CDF and LEP detector groups, which were comparable in size, complexity, and expense of previous accelerators, ironically reversed some of the trends begun in the mid-1960s in response to increasing

---

<sup>41</sup>Panel session.

<sup>42</sup>Maurice Goldhaber, “The Beginning of Program Committees.”

<sup>43</sup>A measure of the rising complexity of experimental proposal procedures can be taken from the growing number of pages needed to describe them. A 1974 Fermilab handbook had 3 pages of description; the 1979 handbook had 11 pages of description. See National Accelerator Laboratory, “Procedures for Experimenters,” 1974 and Fermilab, “Procedures for Experimenters” 1975-1979.

scale and tightening budgets. These giant collider projects, which gathered several hundred physicists working in dozens of groups from facilities in U.S., Europe, and Japan, helped to dilute the influence of outside users in experiments (though not necessarily in laboratory decision making) in both Europe and the U.S.. In both cases, detector collaborations were formed around a core of powerful inside users, who were in a prime position to oversee the efforts of the scattered collaborators and coordinate their work with the activities of the host laboratory. Since a project needed a wide base of enthusiastic support to obtain funding in the late 1970s due to the increasingly unfavorable science policy climate in Washington, large laboratories faced a new struggle to balance the needs of inside and outside users.<sup>44</sup>

Changing scale had other affects on experimentation in the U.S. Greater technological complexity of detectors and other experimental apparatus led to increased reliance on systematic problem solving and engineering skills and decreased emphasis on frugality. In addition, as Ramsey noted, the immense size of collaborations gave rise to a more hierarchical organizational structure and formalized procedures for intracollaboration communication and decision making.<sup>45</sup>

In the era of charm physics, computing brought particularly profound

---

<sup>44</sup>Krige has noted that the struggle between outside and inside users hinges on “conflicts over ownership and control. The form taken by those conflicts will vary depending on the context. The substance will persist” as long as laboratories exist. John Krige, “The Relationship Between CERN and its Visitors in the 1970s,” to appear in John Krige (ed.) *History of CERN, 1965-1980*, (Amsterdam: North Holland, 1994). Research at CERN during this period will be described in this volume, The transformation of research in the 1970s and 1980s at Fermilab is described in Catherine Westfall, Lillian Hoddeson, Mark Bodnarczuk, and Adrienne Kolb, *Fermilab, 1965-1990: A Case Study in the Emergence of Big Science* to be published.

<sup>45</sup>As Krige noted, the organization of research and the influence of engineers did not change much at CERN, which had traditionally favored tightly organized experiments and solid engineering. See Part II of this essay for more discussion on the implications of the differences between the U.S. and CERN styles. Lillian Hoddeson pointed out that in the late 1970s and early 1980s Fermilab also developed a more formal approach to team organization and a more careful, meticulous approach to building apparatus when faced with the technological challenge of developing superconducting magnets. Lillian Hoddeson, “The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972-1983,” *Historical Studies in the Physical and Biological Science*, 18, 1987, pp. 25-54. Also see Peter Galison, “Probe Report on History of the Psi Experiment,” *AIP Study of Multi-Institutional Collaborations: Phase I: High-Energy Physics*, Report 4 (New York: American Institute of Physics), 1992, p. 81.

changes to experimentation. As apparatus became more complex, the amount of data grew, and the need to share data among groups increased, high energy physicists relied more and more heavily on the computer. As Peter Galison has noted, around the mid-1970s the growing importance of computing restructured the organization of research. Whereas previously work was divided into two, sequential steps, detector building and data analysis, subsequently provisions were also made for “a third axis of work differentiation around computer programming, spanning the full cycle of data acquisition, maintenance, distribution, and analysis.”<sup>46</sup> Computing also increasingly dominated the attention of researchers. As a result, in Kowarski’s words, “the idea of an experiment” shifted “from the setting up and running of apparatus to the reduction and analysis of data.”<sup>47</sup> In Galison’s opinion, this shift “may be the sea change of twentieth-century experimental physics.”<sup>48</sup>

## The Wave of the Future

Panelists expressed considerable worry about the future of large laboratories, since troubling trends in the 1964 to 1979 period have accelerated, some previous solutions no longer seem viable, and new challenges have arisen. The chronic funding difficulties experienced by the multi-billion dollar Superconducting Super Collider (SSC), which has faced possible cancellation on several occasions in the early 1990s, dramatically illustrate that since 1980 large laboratories have been squeezed more firmly than ever before by tight budgets and the inevitable cost increases that accompany growth in scale. The strategies devised to overcome this problem in the 1970s – creative financing and a quick, frugal, but risky accelerator building style – are of limited utility to those building the SSC, who face a sometimes hostile reception in Washington, a funding agency that demands exacting accountability, and very large scale technology that can best be implemented with careful planning, reliable engineering, and the help of industry. To accommodate government requirements and industry’s new role as a full partner in the

---

<sup>46</sup>Galison, Note 44, p. 80.

<sup>47</sup>As quoted in Peter Galison, *How Experiments End*, (Chicago: University of Chicago Press, 1987) p. 151.

<sup>48</sup>Galison, note 46, p. 151. See Part II of this essay for other examples of changes accompanying the increasing scale of computing.

construction phase, SSC leaders have been forced to invent new approaches to accelerator building, especially for the organization and management of the project, and simultaneously overcome daunting technological hurdles. At the same time, they have gone to battle in the media, on the floor of Congress, with the Department of Energy, and within the physics community to justify the cost and relative value of the facility. The difficulty of these efforts has underlined the importance of devising better procedures for adjudicating competing funding claims for scientific research.

SSC planning has also prompted new concerns about future modes of experimentation. Since the new laboratory, if built, will have 1000-member groups working for over a decade on a single experiment, high energy physicists have worried about the difficulties of training graduate students, recognizing the contributions of junior collaborators, and encouraging scientific creativity and productivity at this scale of experimentation.<sup>49</sup> Other observers have questioned whether deception, error and fraud are more likely to occur in such massive collaborations, due to the difficulty of identifying individual responsibility. Another worry is that the informal nature of large teams will undercut efforts to ensure fair treatment for all members, regardless of race, religion, age, and gender.<sup>50</sup>

Perhaps the most troubling aspect of the future of large laboratories is the continuing deterioration of the relationship between government and science. Panofsky complained that every time a mistake is made by one individual within any one large laboratory, all laboratories are burdened with "another layer of oversight and criticism is leveled at the entire profession of scientists." One result is that laboratories are faced with "ever-increasing

---

<sup>49</sup>High Energy Physics Advisory Committee, "Report of the HEPAP Subpanel on Future Modes of Experimental Research in High Energy Physics" (Washington D.C.: U.S. Department of Energy, 1988); High Energy Physics Advisory Committee, "Report of the HEPAP Subpanel on High Energy Physics and the SSC Over the Next Decade" (Washington D.C.: U.S. Department of Energy, 1989); American Institute of Physics, *AIP Study of Multi-Institutional Collaborations: Phase I: High-Energy Physics*, pp. 31-32. In "Some Socio-Historical Aspects of Multi-institutional Collaborations in High-Energy Physics At CERN Between 1975 and 1985," John Krige acknowledges the concerns accompanying the increase in scale but argues that the difficulty perceived by physicists is largely due to the tenacious myth of the lone scientific genius, not the actuality of work at greater scale.

<sup>50</sup>Jeffrey Stine, "Edited Excerpts from a Smithsonian Seminar Series," *Knowledge Collaboration in the Arts, the Science, and Humanities*, (Washington D.C.: Smithsonian Institution Press, 1992), pp. 400-406.

pressures for more prior approvals, prior repeated cost analyses and cost reviews,” in short, detailed justifications and formal approvals for every step in the research process. Such practices prompt concern about the productivity of large laboratories.<sup>51</sup> As Seidel has warned, “The capabilities of [large] laboratories ... are rich, but they are also easily stifled by the dead weight of a regulatory bureaucracy. A balance must be struck between responsibility and freedom in big science if it is to be a productive enterprise.”<sup>52</sup>

Both Seidel and Panofsky felt that the problems of the 1980s and 1990s raised questions about the future of the relationship between government and science. Since he finds a close link between the development of accelerators and national security considerations, Seidel questioned whether the government will be willing to support a project as expensive as the SSC now that the Cold War is over, especially since prominent scientists opposed the development of major military projects, such as the Strategic Defense Initiative. Although Panofsky disagreed with Seidel’s interpretation, he agreed about the uncertain future of large laboratories. “We are seeing a shift from the partnership between government and science,” he explained, to “‘acquisition’ of science by government,” an approach non conducive to creative problem solving and the advancement of scientific knowledge. “Nothing short of restoring a spirit of mutual trust and confidence between government and the scientific community can reverse” the trend and reconcile the partners so that they can continue to accomplish mutually beneficial goals.<sup>53</sup>

Prospects for the future are not entirely gloomy, however. Krige stressed that CERN was in a good position to prosper in upcoming decades, since the laboratory is an important political symbol and provides a unique resource (aside from DESY) for scientific projects to which European physicists have special access. In addition, governments would find it “extremely difficult to withdraw” support, due to the “enormous diplomatic and political consequences.”<sup>54</sup>

The very development of the Standard Model also inspires optimism. This achievement testifies to the rich dividends that accrue when physicists and their governments make the sometimes risky investments necessary to

---

<sup>51</sup>Panel session and Wolfgang Panofsky, “Round Table Statement.”

<sup>52</sup>Robert Seidel, “Summary of Symposium on Science Policy Issues of Large National Laboratories.”

<sup>53</sup>Wolfgang Panofsky, “Round Table Statement.”

<sup>54</sup>Panel session.

continue the search for the fundamental nature of matter. Although large laboratories face a number of formidable problems, these difficulties are in Ramsey's words, "merely the cost for being able to do one of the most exciting kinds of research known to man."<sup>55</sup>

---

<sup>55</sup>Panel session.

**Panel Session – Part II**  
**Some Sociological Consequences of High Energy**  
**Physicists’ Development of the Standard Model, 1964-1979**

Mark Bodnarczuk  
*National Renewable Energy Laboratory*  
and  
*The University of Chicago*  
and  
*Fermilab History Collaboration*

In a scientific discipline that went from experiments with less electronics than a VCR to  $10^5$  channels and from collaborations with 5-10 members to 300 during the years 1964-1979, the notion of what high energy physics (HEP) is, or what constitutes *being* a high energy physicist, cannot be viewed simply as an immutable category that is ‘out there’ – that remains fixed despite these and other developments. What HEP is as a discipline, and what it means to be a high energy physicist are re-negotiated by participants relative to the experimental and theoretical practices of the field at a given point in time. In this essay, I will explore some of the sociological consequences of the experimental and theoretical decisions made by high energy physicists as they constructed the edifice that has come to be known as the Standard Model.<sup>56</sup>

---

<sup>56</sup>Currently, there are numerous approaches to the social study of science. For a traditional view of the sociology of science see, Robert Merton, *The Sociology of Science, Theoretical and Empirical Investigations*, (Chicago: The University of Chicago Press, 1973). Some of the earliest work in the sociology of knowledge can be found in Karl Mannheim, *Ideology and Utopia; An Introduction to the Sociology of Knowledge*, (New York: Harcourt Brace Jovanovich, Publishers, 1985), and the early development of the “strong programme” of the sociology of scientific knowledge (SSK) is best represented in David Bloor, *Knowledge and Social Imagery*, 2nd ed. (Chicago: The University of Chicago Press, 1991). Some of the more moderate proponents of SSK include Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, (Cambridge: Harvard University Press, 1987) and Trevor J. Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection*, (Dordrecht: D. Reidel, 1986). Perhaps the most radical SSK position is Steve Woolgar, *Science, the Very Idea*, (New York: Tavistock Publications, 1988). More recently, Andrew Pickering has collected a number of essays that focus on the central role of practice in SSK, in Andrew Pickering (ed.), *Science as Practice and Culture*, (Chicago: The University of Chicago Press, 1992), and Stephen Cole has provided the first serious critique, by a traditional sociologist, of the SSK position in Stephen Cole, *Making Science, Between Nature and Society*, (Cambridge: Harvard University Press, 1992).



Happily, many of these physicists' decisions about the Standard Model have already been carefully documented in Andrew Pickering's sociological history of the development of particle physics, as well as numerous chapters from this present volume.<sup>57</sup> I am thinking particularly of factors like the postulation of the notion of quarks and the development of the Eight-Fold Way and S-matrix bootstrap theory; scaling, hard scattering and the 1967 SLAC experiment's evidence for point-like structure in hadrons; the quark-parton model that was supported by experimental evidence for J/Psi, bare-charm, and Upsilon particles; the development of gauge theory, the standard model of electroweak interactions, with the experimental evidence of neutral currents; and finally the development of a theory of strong interactions – quantum chromodynamics. Other than to underscore physicists' decisions to pursue higher and higher energies (as evidenced in the construction of a 200, then 400, GeV proton accelerator at Fermilab), I will not recount these details here. Rather, within the context of such decisions I will attempt to describe how the increases in scale, cost, and complexity mentioned earlier were *consequences of the choices* to go to higher and higher energies in response to the experimental evidence and theoretical constructs that emerged from 1964 to 1979.<sup>58</sup> More particularly, one consequence seen at Fermilab was the development of an increasingly complex and bureaucratic organizational infrastructure that I will characterize below as a number of interrelated

---

<sup>57</sup>Andrew Pickering, *Constructing Quarks; A Sociological History of Particle Physics*, (Chicago: The University of Chicago Press, 1984).

<sup>58</sup>Pickering claims the relationship between experimental and theoretical research traditions is symbiotic in that each generation of practice within one tradition provides a context within which the succeeding generation of practice in the other finds its justification and subject matter. Galison claims that the truism that "experiment is inextricable from theory" or that "experiment and theory are symbiotic" is useless because while vague allusions to Gestalt psychology may have been an effective tactic against dogmatic positivism, experimentalists' real concern is not with global changes of world view. For Galison, the salient issue is where theory exerts its influence in the experimental process and how experimentalists use theory as part of their craft. My point is that once physicists decide to study certain physical phenomena and theoretical constructs at higher and higher energies, such a decision has physical consequences (larger accelerators given the technologies during the 1964 to 1979 era, and larger more heavily instrumented fiducial volumes in apparatus to detect myriad particle interactions) and sociological consequences of the types that constitute the remainder of this essay. See Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics*, pp. 10-11, and Peter Galison, *How Experiments End*, (Chicago: The University of Chicago Press, 1987), p 245.

resource economies, each having its own commodity.<sup>59</sup> Another consequence of larger more complex detectors was the need for larger more complex social structures for the collaborations that designed, fabricated, installed, and operated them, as well as an increased scale and complexity for the on-line and off-line computing power needed to bring data samples to final publication.

After 1972, Fermilab operated the highest energy particle accelerator in the world, and consequently competition for use of the wide variety of particle beams it produced (primary, secondary, and tertiary) was intense. In order to gain access to one of these particle beams, experimentalists had to navigate a number of inter-related resource economies that were embedded within an institutional structure headed by a single scientist, the director, who had ultimate authority in all matters scientific and otherwise.<sup>60</sup> Experimentalists had to learn to trade with and for these commodities in order to participate in the production of knowledge in high energy physics. Physicists negotiated with these commodities and often fought over them.<sup>61</sup>

One economy at Fermilab was proton economics, based on protons as the commodity. The overall magnitude of the economy was limited by such factors as accelerator flux, efficiencies in primary beam transport, cross-sections for secondary beam production, secondary beam transport efficiencies, and expected reaction rates in experimental targets. For example, given the cross-section for neutrino production ( $10^{-36}$  cm<sup>2</sup>) and the pion cross-section ( $10^{-27}$

---

<sup>59</sup>I developed the resource economy model from a case study of several Fermilab experiments. See Mark Bodnarczuk, "The Social Structure of Experimental Strings at Fermilab: A Physics and Detector Driven Model," Fermilab-Pub-91/63, Batavia, March 1990, p 2 ff. This HEP-specific model is not unlike Bruno Latour's more generic model of cycles of credit that involve conversions of different types of capital (recognition, grant money, equipment, data, arguments, articles etc.) into the "credibility" that scientists need to make moves within a scientific field. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, (Princeton: Princeton University Press, 1986), pp. 187-233. Also, Sharon Traweek describes this phenomenon by explaining how laboratories took on some of the features of a market economy.

<sup>60</sup>In Part I Maurice Goldhaber remarks on the early formation of program committees appointed by laboratory directors for the purpose of obtaining independent assessments of the laboratory's research program.

<sup>61</sup>Using numerous case studies, David Hull claims that not only are infighting, mutual exploitation, and even personal vendettas typical behavior for many scientists, but that this sort of behavior actually facilitates scientific development. David Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, (Chicago: The University of Chicago Press, 1988), p 26.

cm<sup>2</sup>), the decision to approve experiments using incident beams of neutrinos was already a major decision that affected proton economics.<sup>62</sup> A neutrino beam was much more costly than a pion beam in terms of the number of protons needed to produce it and this was further complicated if the cross-section for event production in the proposed experimental target was low and the experiment required a large number of particle events to be competitive with previously accumulated world samples. Given the intense competition for protons, beam management issues became very complex, especially in the kind of outside-user-based environment that typified Wilson's philosophy at Fermilab.<sup>63</sup>

A second economy was experimental real estate – here the commodity was possession of an experimental hall at the end of a beam spigot to house the collaboration's apparatus. As detectors became larger and more complex, the lead times needed to assemble and operate apparatus also increased. Consequently, physicists that *were* given a piece of experimental real estate and some beam time tended to move into an experimental hall with the explicit goal of performing that experiment, and the implicit goal of not moving out. Gaining access to an experimental hall, especially when the incumbent collaboration was desperately attempting to hold its place in line, made possession of this commodity one of the most important items to be obtained in an outside-user-based environment.

Another economy, I call “physicist economics,” is based on the commodity of physics expertise. Although the scale and complexity of the experiments during the 1964 to 1979 time period continued to increase at an unprecedented rate, the number of high energy physicists that could commit themselves to perform experiments was constrained by the total number of physicists available at that point in time and the rate at which new Ph.D. graduate students were being produced. Consequently, the enormous increases in the scale and complexity of experiments made physics expertise an

---

<sup>62</sup>The pion cross-section is roughly constant for energies above two GeV at about 25 millibarns. The neutrino cross-section is not constant, but is linearly proportional to the energy. For Fermilab, a reasonable neutrino energy to use was 100 GeV, which would give a neutrino cross-section of about 0.7 picobarns.

<sup>63</sup>In the first part of this essay, Westfall notes how Edwin Goldwasser (who later became Wilson's Deputy Director) was one of the first to address outside user discontent in the United States.

increasingly valuable commodity.<sup>64</sup> Larger more complex, increasingly modularized detectors required larger more complex, increasingly modularized social structures with the appropriate *number* of physicists and the *distribution* of expertise needed to design, fabricate, install, and operate the apparatus and to develop the computing systems and software programs used to reconstruct and analyze the particle events that were recorded. By the late 1970s, collaborations were characterized by an unprecedented division of labor so that no single member of the collaboration had a detailed knowledge of the entire detector. As pointed out by Galison, this kind of modularization provided each institution with a visible manifestation of its contribution to the experiment.<sup>65</sup> Not only was the modularization of detectors an important aspect of carving out a piece of physics to work on, it was also an important political issue back at the home university. Within the economy of physicist economics, proposals were increasingly judged on the “physicist design” of the experiment and how well it mapped to the experimental design, with laboratory directors and their advisory committees focusing more and more on whether the collaboration had enough physicist power to make good on its experimental claims.

But the consequences of physicists’ choices (increased scale and complexity of detectors, accelerators, and the associated social structures) are most easily seen in a fourth resource economy, computing economics, based on the commodity of on-line and off-line computing power. One example was the attempt to do high statistics charm experiments at Fermilab in the late 1970s.<sup>66</sup> On the one hand, the advantages of on-line data reduction using sophisticated trigger processors had to be balanced against the risk of coming up empty handed due to wrong trigger assumptions and the problems of ob-

---

<sup>64</sup>For example, a recent study of the HEP research program for the 1990s performed by the High Energy Physics Advisory Panel, under the auspices of the United States Department of Energy, included a detailed demographic study of “manpower considerations” during the time period under study. See the High Energy Physics Advisory Committee Subpanel, “The U.S. High Energy Physics Research Program for the 1990s,” DOE/ER-0453P, Washington, D.C., April, 1990, pp. 68 ff.

<sup>65</sup>Peter Galison referred to the visibility that modularization provided participants in his talk at the Third International Symposium, The History of Particle Physics, The Rise of the Standard Model, 24-17 June 1992, Stanford, California.

<sup>66</sup>See Mark Bodnarczuk, note 56, for a detailed case study of Fermilab experiments E-516, E-691, E-769, and E-791 that performed high-statistics photoproduction and hadroproduction of charmed particles.

taining the commodity of off-line computing power. On the other hand, the more secure approach to on-line data acquisition (the write it all to tape approach) had to be balanced against the problem of obtaining immense off-line computing resources, which was difficult given Wilson's belief that the bulk of computing for experiments should be provided by the collaboration's home institutions.<sup>67</sup> There was an abrupt explosion in the number of channels of electronics in detectors after 1980. In terms of the magnitude of computing and channels, the time period during the development of the Standard Model was the calm before the storm – before the explosion in scale, cost, complexity of hadron collider detectors (like CDF) that were conceived after 1977.<sup>68</sup>

A final resource economy was physics economics; the commodity of published physics results was traded back to the laboratory director as a return on investment and was the key to obtaining additional resources to perform follow-up experiments. Within the economy of physics economics, the laboratory director's ability to approve or disapprove an experiment was a powerful management tool for leveraging way-ward experimenters who failed to make good on their physics promises, especially when they wanted to move on to the greener pastures of follow-up experiments without first publishing their results.

The study of various Fermilab experiments mentioned earlier also shows that within this socio-economic-scientific infrastructure of the laboratory, experiments were performed in series of follow-up experiments in which an experiment was performed, then transformed into a second, then a third, or a fourth experiment. I call these series of experiments "experimental strings."<sup>69</sup> Key to describing these transformations is the ability to characterize the continuities between individual experiments in such strings. Evidence that

---

<sup>67</sup>Mark Bodnarczuk interview with Robert Wilson, 24 September 1992.

<sup>68</sup>The UA1 detector at CERN had about 50,000 channels, the Collider Detector Facility (CDF), the D0 Collider at Fermilab, and the SLD detector at SLAC each had about 100,000 channels, the ALPEH detector at LEP had about 700,000 channels, and the proposed SDC and GEM detectors at the SSC may have as many as 50,000,000 channels depending on the available technology.

<sup>69</sup>See Mark Bodnarczuk, note 56, p. 14 ff; see Joel Genuth "Historical Analysis of Selected Experiments at US Sites," *AIP Study of Multi-Institutional Collaborations: Phase I: High Energy Physics*, Report 4, (New York: American Institute of Physics, 1992; Fredrik Nebeker's unpublished manuscript, "Experimental Style in High-Energy Physics: The Discovery of the Upsilon Particle," January, 1993.

emerged from the previously mentioned study suggests that these experimental strings exhibit well-defined continuities in the physics goals, the detector configuration design, and in the core group of collaborators that participated in 9 or more experiments over a 20 year period spanning three laboratory directors.<sup>70</sup> These continuities transcend a single experiment and provide a method for understanding more complex social structures and research programs that exist for more than 15 years. Each experimental configuration in a string displays a more complex iteration of the original apparatus which leaves the fundamental design of the modularized detector sub-systems largely intact. In other words, experimental strings are like mini-institutions within the organizational infrastructure of the laboratory. People outside the laboratory really do not know about them because they do not have formal names.

I believe the experimental string is the preferred and more interesting unit of study for sociological and historical analysis because the numbers that laboratories like Fermilab assign to experiments are not at all indicative of what

---

<sup>70</sup>The major fixed-target experimental strings at Fermilab were the E-82, 226, 383, 425, 486, 584, 617, 731, 773 string, the E-531, 653 string, the E-8, 440, 495, 555, 620, 619, 756, 800 string, the E-21A, 262, 320, 356, 616, 770 string, the E-594, 733 string, the E-98, 365, 665 string, the E-1A, 310 string, the E-95, 537, 705, 771 string, the E-70, 288, 494, 605, 608, 772, 789 string, the E-87, 358, 400, 401, 402, 687 string, and the E-516, 691, 769, 791 string. By way of comparison with fixed-target counter experiments, the major continuity between the experiments performed with the 15-foot bubble chamber at Fermilab (experiments E-28A, 31A, 45A, 53A, 155, 172, 180, 202, 234, 341, 380, 388, 390, 545, 564, and 632) seems to be the chamber itself. In a less defined way, there were some continuities in the target substances with which the chamber was filled. But the social structures of these collaborations were different from the fixed target counter experiments. Bubble chamber spokespersons seemed to draw upon the expertise of the international community of bubble chamber physicists each time they formed an experimental group and consequently the collaborations did not exhibit the same type of well-defined core-group structure found in large, complex fixed-target counter experiments. My preliminary studies show that the relatively non complex social structure of these collaborations results from the existence of a Fermilab-based Bubble Chamber Department devoted solely to the operation and maintenance of the complex systems of the chamber, independent of the collaborations that use it. This type of heterogeneous Fermilab/collaboration social structure with a dedicated support group is not evidenced in even the largest fixed-target counter experiments at Fermilab, but it is interesting to note that a similar phenomenon (dedicated support departments) does appear with the advent of the mammoth collider detectors like CDF and the D0 detector. For historical details see Mark Bodnarczuk (ed.), *Reflections on the Fifteen Foot Bubble Chamber at Fermilab*, (Batavia: Fermilab, 1989).

actually constitutes “an experiment.” Actually, the experimental numbers assigned by laboratory management are more indicative of such factors as the laboratory’s accounting practices, the bureaucratic steps involved in the approval process as defined by a particular director, funding scenarios both inside and outside the laboratory, contrasts between the in-house-facility approach to doing experiments (where strings were largely determined by the laboratory management), and the outside user-based-non-facility approach (where institutions came together and formed strings more voluntarily). But these numbers do not define what an experiment *is*.

While it has been common practice for philosophers, historians, and sociologists of science to “extract” an experimental “case study” from the organizational infrastructure of the laboratory in which it was performed and attempt to study it as a stand-alone unit, the fact is that experiments like those performed at Fermilab did not exist independent of the organizational infrastructure of the laboratory in which they were embedded. Experiments/collaborations were not closed systems, cohesive entities, or “objects” that had unambiguous boundaries and could be divorced from the dynamics of laboratory life.

Of course experimentalists did attempt to draw a firm line of demarcation around the “collaboration” or “experiment” and its activities for the sake of defining which names appear on scientific publications, but the fact is that numerous laboratory personnel often play crucial roles in experiments, and whether or not their names appear on the published paper is a socially negotiated matter that is decided by the personalities involved.<sup>71</sup> Attempts to “map” the names on various experimental proposals (or the resultant publications) to the collaboration members who actually performed the day-to-day tasks associated with the experiment show that the names on proposals or papers are often not indicative of who actually performed the work of the experiment. Names of individuals who did not play any substantive role in a

---

<sup>71</sup>Melvin Schwartz shows how tenuous these socially negotiated walls are for today’s large collaborations when he advocates divorcing some of the detector builders from the collaboration, then sub-dividing the remaining members of these mega-collaborations into distinct (smaller) collaborations that would develop their own research programs and compete for time using the detector. In a sense, Schwartz is advocating a return to a social structure that is not unlike that displayed in large bubble chambers as I described in the previous note on the fifteen foot bubble chamber at Fermilab. Also, see Faye Flam, “Big Physics Provokes Backlash” *Science*, 30, 11 September 1992, p. 1470.

particular experiment are included on a proposal or the physics publications because, in some cases, that individual may have had major responsibility for constructing a portion of the detector in an earlier experiment in the string, or because they have committed a fraction of their overall professional time at the proposal stage, but never come through on these commitments because of the heavy load of administrative duties at their home institution, the host laboratory, or because of commitments to other experiments that they perceived were producing more important physics results.<sup>72</sup> This is probably related to the problems associated with “physicist economics” and is a fruitful issue for future sociological research.

Physicists’ choices to go to higher and higher energies in response to the experimental evidence and theoretical constructs that emerged during the 1964 to 1979 era, and the effect that this had on increasing scale, cost, and complexity, reveal interesting contrasts between the American and European (CERN) style of doing physics. During the 1964 to 1979 time period, many American physicists preferred the more informal, non-bureaucratic, quick and dirty, frugality style of doing physics.<sup>73</sup> But the European approach was typified by what American physicists considered to be an overly formal, inflexible, bureaucratic, over-engineered, gold-plated approach to doing physics. Even after the mammoth collider detectors began to be conceived in the late 1970s, both American and European physicists were relatively unreflective about the role that social factors were beginning to play in HEP. And consequently, the sociological challenges that were intrinsic to collider detector environments with  $10^5$  or more channels received little or no systematic study by practicing physicists. The sociology of large collaborations just wasn’t viewed as a part of doing HEP and like the policy of physics journals, the social and human factors were just *left out*.

But despite this lack of conscious self reflection on both sides of the Atlantic, the values embodied in European culture more naturally gave rise to a style of physics that was more formal in terms of well defined roles,

---

<sup>72</sup>Some collaborations (like CDF) required members of the collaboration to run a certain number of data taking shifts in order to have their name on publications, but many collaborations had no such policies.

<sup>73</sup>In Part I of this essay, Westfall refers to a similar type of non-bureaucratic, quick and dirty, frugality style at SLAC. Also see Catherine Westfall and Lillian Hoddeson, “Frugality and the Building of Fermilab, 1960-1972,” to be published in “Science and the Federal Patron,” Nathan Reingold, and David van Keuren, ed.



responsibilities, and authorities for physicists and engineers and was more focused on producing robust engineering and physics designs that were less flexible in terms of programmatic changes. As it turned out, these were the very practices, values, and beliefs that became *crucial* to mounting mammoth collider detector experiments.<sup>74</sup> Conversely, the less formal approach to doing physics put American physicists at a disadvantage in terms of confronting the kinds of organizational and management problems that emerged from this enormous growth in scale, cost, and complexity. While the American style of doing physics may have been an advantage with the scale, complexity, and costs typified by the detectors in most of the 1964 to 1979 period, it became a crucial disadvantage for experiments conceived in the late 1970s (CDF, D0, and the LEP detectors), and is absolutely terminal for detectors of the scale, complexity, and cost of the proposed SDC and GEM detectors.<sup>75</sup> Also, the European style of doing physics allowed a more natural transition from the smaller experimental scale that typifies the 1964 to 1979 era, to the SSC detectors that are now the scale of SLAC. In the SSC detector environment, not only can social factors no longer be left out of any salient definition of what HEP is, social factors become one of the most crucial aspects of doing HEP – they could even become *the* limiting factor of the future of the field.

---

<sup>74</sup>Kevles attributes the scientist's tendency to leave social factors out of their accounts of science to being accustomed to the literary convention of journal editors and the fact that many scientists consider themselves to be incompetent to write about anything except science itself. Daniel Kevles, note 7, p. xiv. Pickering claims that references to "judgments" or "agency" on the part of scientists are left out of scientists accounts so that scientists are portrayed as passive observers of nature, with experiments appearing to be the supreme arbiters of competing theories. Pickering, note 54, p. 5 ff. Latour claims that there are definable processes that operate to remove all aspects of the social and historical context in order that scientific "facts" do not appear to be socially constructed, see Latour and Woolgar, *Laboratory Life: The Construction of Scientific Facts*, p 176 ff. and Latour, note 53, p. 22 ff.

<sup>75</sup>It is interesting to note that in his address in honor of the 75th Anniversary of the Max Planck Institute for Physics in Munich Germany, James D. Bjorken devoted a major portion of his visionary article to the problems associated with the sociology of large collaborations and the possibility that these social factors might have an effect on the physics itself. See James D. Bjorken, "Particle Physics – Where Do We Go from Here?" SLAC Beam Line, 22, Winter, 1992, p 10.